Associate Editor decision: Publish subject to minor revisions (review by editor)
by Fiona Clubb
Comments to the author:

Dear authors,
I have now received your revised manuscript, response to reviewer's comments, and re-review comments from Reviewer 1. I would like to thank you for engaging constructively with the reviewers. The new manuscript is significantly improved from the initial version, with the novelty of the experimental approach and the importance of the topic for our understanding of landscape evolution clear. I think this paper will be of great interest to the geomorphology community and is a fitting contribution to ESurf. I am happy to accept your manuscript for publication in ESurf following some minor corrections to address which are detailed below.

Best wishes,
Fiona

Thanks for your kind comments.
We explain below the latest corrections we have made in response to your comments and re-review by reviewer 1.

General comments

1) Both reviewers noted issues with style and language. Some of these have been addressed through the revision process, but there are still some issues throughout which make it hard to understand some of the text. Please carefully read through your manuscript and do a check for clarity. I have made some specific suggestions below and suggest that you address the handwritten comments from Reviewer 1's re-review (although a line-by-line response to these is not needed when submitting your revision).

We hope that the new corrections will have improved these issues.

2) Reviewer 1 raised some good points about the retreat rate patterns that you observe during the experiments. I agree with the reviewer that the pattern of knickpoint retreat acceleration followed by deceleration are clear in Experiment BL15, but not clear from the other experiments. In their initial review the reviewer asked for more robust statistical testing, which was not addressed in the revised manuscript. Please address this comment by testing the significance of the modelled quadratic fit to the observed knickpoint retreat rates for each experiment
.
Done. We modified the related figure (Fig. 9) to better illustrate the acceleration and deceleration trends in experiments BL05 and BL10 (see also comments below). We have also added the statistical tests suggested by reviewer 1 in a new supplementary figure (now Fig. S4 in the Supplemental Material).

Specific comments:

Figure 1C: please annotate this photo with the position of the sliding gate at the outlet, to make it clear to the reader which orientation the box is in in this photograph.

Done.

Extraction of river long profiles from the experiments: only one river profile is extracted from each DEM, but from the photos in Fig 1 it looks like there are multiple catchments within each experimental setup. It's not clear how the "main river" was identified from the experiments. Please clarify in the methodology.

Done. We considered one river per experiment, generally the one with the largest catchment.

Figure 6: please avoid the use of the Jet colourmap. I suggest using one which is more perceptually uniform (e.g. viridis, plasma, etc.).

Done, we now use the Plasma colormap in Figure 6.

Technical corrections:

Line 21: typo "heir"

This was corrected

Line 66: should be "evolve" rather than "evolved"

This was corrected

Line 89: missing space between Table and the number

This was corrected

Throughout the text there are a few times when an extra "s" is added to "knickpoint". E.g. Table 1, Fig 10 caption. Please check for grammar.

This was corrected

Line 133: Fix sentence structure.

This was corrected

Figure 3 caption is very long and quite confusing: please condense and rephrase.

This was corrected. We have shortened the caption

Line 161: should be "latter" rather than "later"

This was corrected

Figure 7 Caption: Remove the word "Detail".

This was corrected

Reviewer #1.

These are my second round of comments on the submitted manuscript by Lavaissière and colleagues. As I stated in my original review, I am supportive of the publication of this manuscript in ESurf as I think the authors have performed interesting and innovative experiments and analyses that will improve the community's understanding of how knickpoints form and retreat, and what influence this may have on landscape evolution. I also believe the manuscript has improved significantly since its original submission.

In this revised version, the associated editor asked me to specifically discuss 1) the additional discussion about the mechanisms of knickpoint initiation and retreat, and 2) the testing whether a quadratic function is an appropriate fit to the retreat rate curves, and, both of which I commented on in my original review.

Regarding point number 1 above, I believe the authors have (more or less) suitably addressed my comments, and the additional details on the mechanism of knickpoint formation and retreat is appropriate. I think there is still some minor work to do here. I found Section 4.2 where the authors explain many of these details to be a bit hard to follow. In particular, the authors argue in Section 4.2 that 'rivers no longer incised' (L381) after the passage of knickpoints. Figure 11C does a nice job of showing that this is not true. While there are a few cases after the passage of a knickpoint where the river alluviates, the more common trend is for erosion rates to drop below the rate of base level fall, thereby allowing the profile to steepen so that a new knickpoint can be made. I would encourage the authors to change the wording throughout the section to highlight the importance of erosion rate falling below the rate of base level fall (as opposed to erosion rates dropping to zero), as I believe this is the primary mechanistic control.

→ Done. The related sentence in section 4.2 :

*"immediately after the retreat of a knickpoint, we show that erosion is inhibited downstream and **rivers no longer incised** despite the ongoing base level fall, until the passage of a new knickpoint."*

was modified to

"Immediately after the retreat of a knickpoint, we show that erosion in the section of the channel where the knickpoint just passed is inhibited despite the ongoing base level fall: river incision is lower than the rate of base level fall, until the passage of a new knickpoint"

A second point the authors can make here is that it appears the majority of the erosion in the experiments is made by the upstream propagation of autogenic knickpoints, highlighting how important it may be to understand the dynamics of these systems to predict landscape evolution.

This is indeed an important point that we put forward in the conclusion:

"Rivers in our experiments thus evolve following sequences of width widening and narrowing that drive the initiation and propagation of successive knickpoints. As a result, incision is fundamentally discontinuous over time despite continuous forcing. It occurs during discrete events of knickpoint propagation that allow the rivers to recover from the incision delay accumulated during widening periods."

Related, in Section 4.3, the authors seem to contradict themselves. The authors suggest that the bell-shaped curve of knickpoint retreat vs distance (Fig. 9) may be a diagnostic characteristic of autogenic knickpoint creation. However, the authors then go on to argue that their mechanism is analogous to a system with discrete pulses of base level fall (L503-507). I agree, but if that's true, then we would also expect discrete cases of base level fall to produce the bell-shaped curve of knickpoint retreat vs. distance, which I don't think has been previously observed. To me this suggests that there's something else going on that is unique to the experiments here that may be creating this interesting retreat pattern. If the authors can't fully explain why this retreat pattern emerges, I think that's OK and the paper should still be published, but I would encourage the authors to try to revise the text to avoid this contradiction.

We disagree with this comment. We explain in the ms that despite continuous base level fall in our experiments, the geometry of successive longitudinal profiles is similar to the geometry observed when a geomorphic system is forced by discrete drops of base level fall, (section 4.3):

"… Thus, the sequential evolution of longitudinal profiles is very similar to the geometry that would be observed if the system was forced by discrete drops of the base level, rather than by a continuous drop as it is actually the case."

but this similarity of geometries does not imply that the dynamics of knickpoint migration is similar in both cases as the reviewer states. We disagree with the reviewer when he/she writes: "*then we would also expect discrete cases of base level fall to produce the bell-shaped curve of knickpoint retreat vs. distance*" and we have never written nor suggested that in the ms.

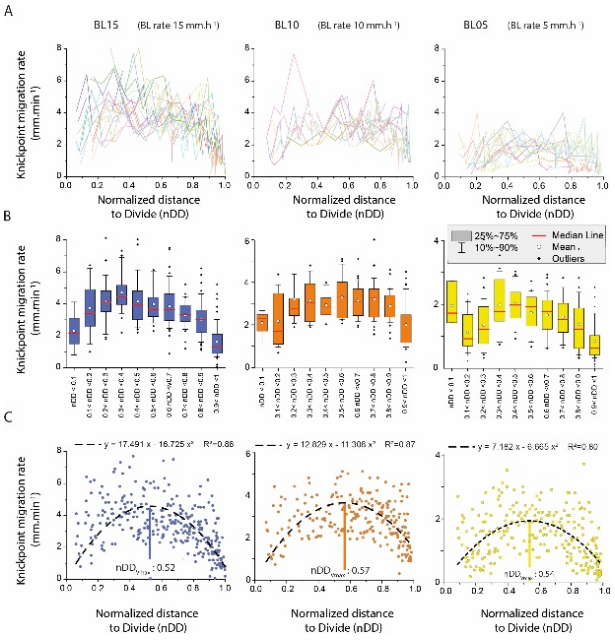
Regarding point number 2, I do not think the authors response and subsequent changes to the manuscript are sufficient. The results from Experiment BL15 are fairly clear that knickpoint retreat rates speed up and then slow down. I do not think this is as obvious in Experiments BL10 and BL05. The authors suggested that their hypothesis of knickpoints speeding up and then slowing down is verified by the fact that the mean and median value of knickpoint retreat show a trend of increasing and then decreasing as knickpoints retreat upstream; however, the change in the mean and median value of knickpoint retreat with respect to distance is small in BL10 and BL05 relative to BL15. If the authors want to argue this trend occurs in all three experiments, I still believe it would be better to do a statistical test which shows that when fitting a line to the retreat rate data vs distance (for 0<ndd<0.55 and 0.55<ndd<1),

the slope of the line is statistically distinct from a slope of 0. I don't think this is a huge deal overall, but it seems like the right thing to do to present a rigorous analysis of the data and to further support the subsequent discussion and interpretations made by the authors.

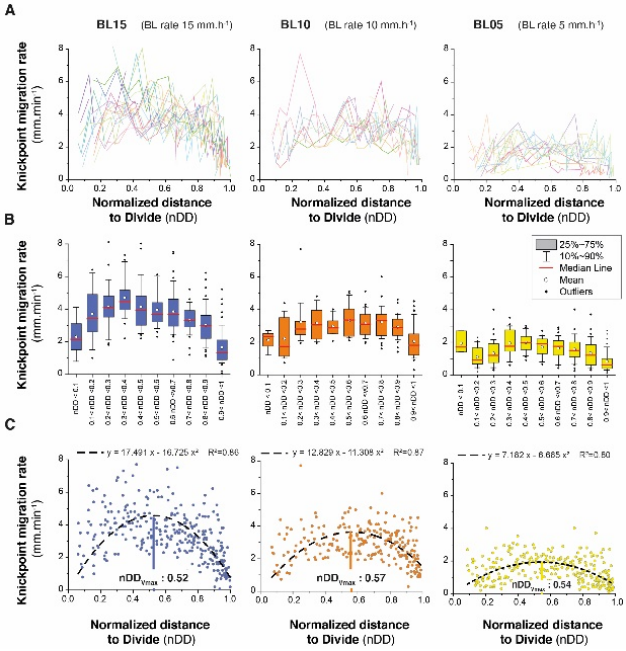
We had thought during the previous revision that it would sufficient to add the statistics on knickpoint retreat rates in Figure 9. As a remined all the statistical data shown in the Figure 9B were not in the initial ms submission.

In fact, the main reason why the trends are not as clear for the low base level fall experiment (BL05 and potentially BL10; cf Fig 9) as for the high rate one is mainly a matter of scale on the graphs. We initially decided to show all the graphs with the same scale on the y-axis to illustrate the fact that migration rates also depend on base level fall rates. We have now modified again this figure by keeping the same scale on the y-axis only in Fig. 9A but by adjusting it to each data range for each graph in Figures 9B and 9C.

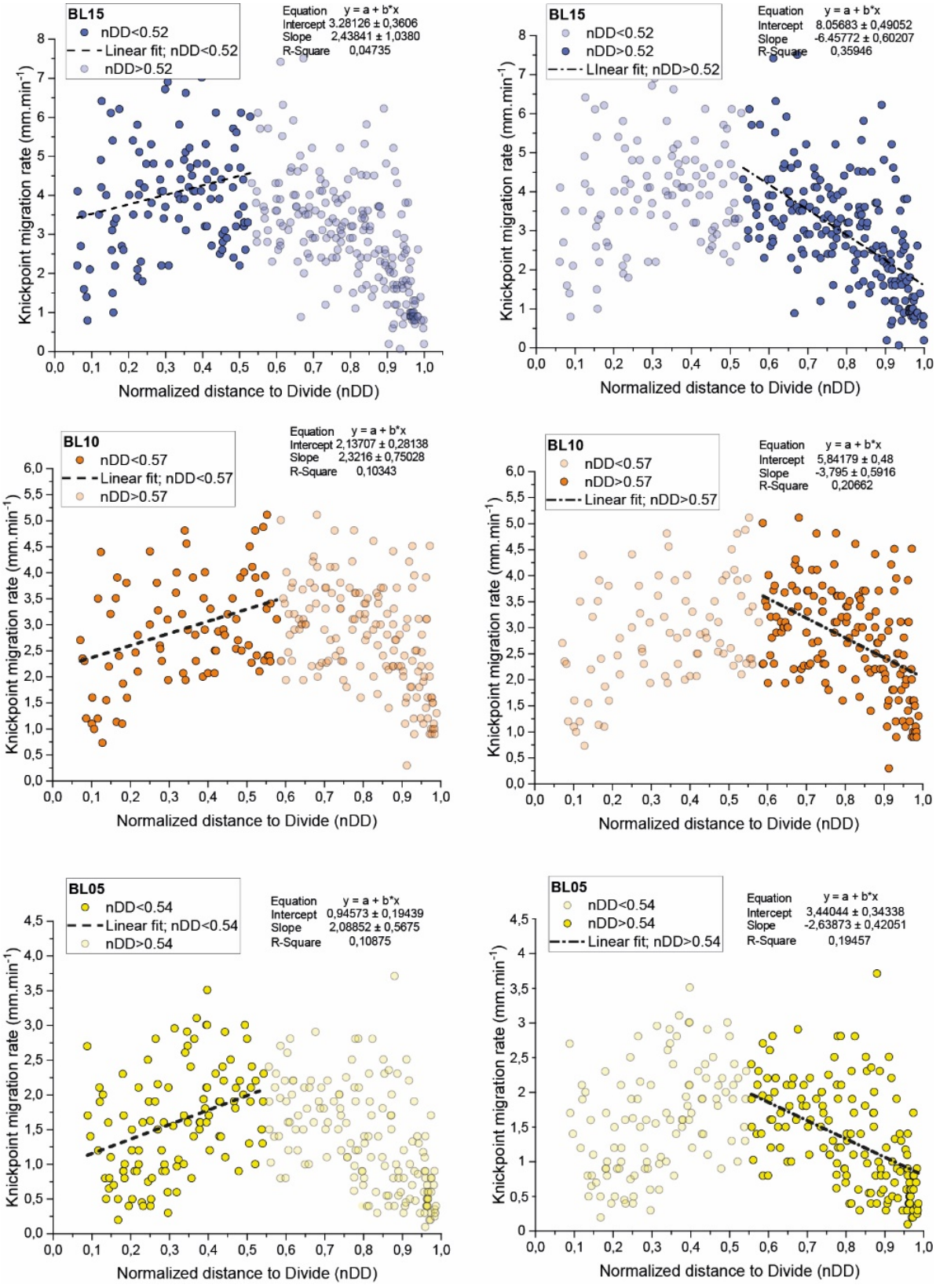
New Figure 9:



Former Figure 9:



We also now provide the statistical test required by the reviewer in a new Supplementary Figure (Fig. S4).



I have a number of other minor to moderate comments that I would like the authors to consider:

1) When reading the manuscript I found several English language errors and other word choice that made it difficult to understand what the authors meant. In many other cases there was not sufficient information given about methods or other details, that I think may limit future readers' ability to fully understand the message the authors are trying to convey. I have annotated the PDF with handwritten comments correcting these mistakes and offering other minor comments throughout the text, and attached my comments to this review. I do not expect a line-by-line response to my handwritten comments, but have included them here for the benefit of the authors. I believe these comments will help make a clearer, easier to read manuscript. I think a careful review of the manuscript by the authors to make sure everything is explained in sufficient detail for a reader to understand the analysis and to ensure the wording is clear is necessary before the manuscript can be published.

We sincerely thank the reviewer for his/her efforts to help improve the manuscript. All suggested corrections have been done.

2) I think, but I am not sure, the authors are defining knickpoints two different ways, but only one is listed in the methods. Figure 2 and associated text (L100-105) explains how the authors calculate knickpoints as a single point (the triangles in Fig. 2). However, in subsequent figures, the authors appear to calculate knickpoints as a **zone of discrete length** (e.g., the hatching of K1 and K2 in Fig. 11). It's not clear how this zone is defined. Does this correspond to the circles in Figure 2? Please make this explicit. Similarly, in Figure 7, it's not clear how knickpoint slope is calculated and over what spatial scale this calculation has been made.

We do not understand this comment, hatched areas in Fig 11 do not correspond to lengths but to time! Figure 11 does not show longitudinal profiles but a whole series of parameters calculated along a channel section. We have defined knickpoints in only one way, as described in the methods.

In Figure 7, the slope is calculated from the difference in altitude between the knickpoint lip and knickpoint base, over its extent.